

belts of a brownish-grey tint; sometimes, though rarely, approaching a rose colour. Often the brown is entirely wanting, and the belts are merely dull streaks on a light yellow ground.

"We can best convey an accurate conception of the colouring, seen on the 5th of January, by stating the manner in which we tinted the drawing, which, when compared repeatedly at the telescope by myself and others, was found correctly to give the colours and their relative intensities.

"The whole disk was first washed over with a slight tint of yellow. The colour of the region between the equatorial belts was correctly reproduced by pure yellow mixed with crimson-lake, while the two dark belts north, and the one south of the equator were obtained by the same combination of colours, with more crimson-lake added, so as to bring the tint to approach a coppery hue. The polar shadings were correctly given by pure yellow with a slight dash of crimson-lake, which tinting was subsequently overlaid with a very thin wash of light lead colour."

Reading over the last paragraph, one might imagine it to be a description of the last coloured drawing which I have made of *Jupiter*. A copy of this drawing forms the frontispiece to Mr. Proctor's new work, *Other Worlds than Ours*. On referring to this coloured plate, the resemblance may be almost as clearly traced as by examination of the drawing. Professor Mayer used a *refractor* of six inches aperture, made by Alvan Clark.

Clapham, June 30th, 1870.

Further Remarks on the Corona. By Richard A. Proctor, B.A.

At the last meeting of this Society, Dr. Gould, of America, indicated his belief that the trapezoidal corona seen by himself and other observers during the progress of the American eclipse was in fact but the chromosphere seen under unusually favourable circumstances. He added, that the light outside that four-cornered corona appeared to shift in position, and hence he concluded that it was terrestrial.

It seems to me that if this view be admitted, the difficulty pointed out by Mr. Lockyer in the case of the corona considered generally, exists in scarcely diminished extent in the case of this trapezoidal appendage also. Estimated by most of the observers as extending fully 12' from the disc of the eclipsed Sun, its real depth would be far more than 320,000* miles, and the pressure even at the summits of the highest prominences would be enormous.

We gain nothing, then, by Dr. Gould's supposition; though of course that does not prove it to be erroneous. But Dr.

* Even in Mr. Whipple's photograph it has an extent of fully 6', which would correspond to more than 160,000 miles, or 80,000 miles above the highest prominences yet seen.

Curtis (whose successful photographs appear in Commodore Sands's reports of the total eclipse) remarks that he has read Dr. Gould's statements respecting the eclipse with considerable surprise. After referring to the photographic evidence, he adds, "Dr. Gould adduces as an additional argument in favour of his assumption the observation that the long coronal beams appeared to him to be 'variable,' while the 'aureole' photographed was evidently 'constant' during the time of totality. This argument, however, loses some of its force, when it is remembered that to other observers the corona appeared to the eye absolutely unchangeable, both in form and position, during the whole period of the total obscuration." He goes on to indicate the probability that Dr. Gould has mistaken a photographic effect for a real phenomenon, in this case, precisely as when he interpreted the apparent encroachment of the bases of the prominences on the Moon (a dark-room phenomenon, as Curtis shows) to "specular reflection" at the Moon's surface.

I must confess, that after a very careful study of the whole series of American observations, Dr. Gould's view appears to me to be altogether disposed of by the concurrent testimony of so many and such skilful observers.

One striking, and as yet unnoticed, piece of evidence exists in General Myers' report of the appearance of the corona as seen from the summit of White Top Mountain, 5530 feet above the sea-level. Here the same quadrangular aspect was observed as at lower levels (and in Whipple's photograph), but the rays were much longer. "The silvery rays," he says, "were longest and most prominent at four points of the circumference—two upon the upper, and two upon the lower portion—apparently equidistant from each other, and at about the junctions of the quadrants designated as 'limbs,' giving the spectacle a quadrilateral shape." He remarks that these silvery rays were "straight and massive," and extended "to a distance of two or three diameters of the lunar disc." He adds, "There was no motion of the rays."

It seems impossible to mistake the significance of these observations.

In my paper in the March number of our *Notices*, I dealt specially with the theory that the corona is due to the illumination of the Earth's atmosphere by light not affected "by any action at the Moon." Many of the arguments, however, apply equally well on the supposition that there is such action. The striking fact that at the time of central eclipse the cone within our atmosphere bounded by lines from the observer's eye to the Moon's limb, contains no light, while the cylinder within our atmosphere bounded by lines from the Sun's limb to (and produced beyond) the Moon's, contains much light, affords, I take it, absolutely convincing evidence that this light is derived from an object far beyond the Moon. For if we suppose the solar rays to get by any process within the cylinder, they should

clearly traverse the cone also. For example, assuming that a solar ray passing by the Moon's edge is deflected (by whatever cause) so as to fall within that cylinder into which (from its very nature) undeflected rays cannot pass, the deflection, in order to account for observed appearances, must carry the illumination of our atmosphere up to the above-mentioned cone, and there suddenly the illumination must cease. But the cone has no existence in nature; it is but a mathematical conception: why then should these deflected rays respect it?*

Even La Hire's theory, which De Lisle is supposed to have overthrown, seems more easily supported than one which requires a moving shadow-cylinder of air to be illuminated, while a fixed cone (*not* a shadow cone) within it remains in darkness.

It seems much more natural to regard the blackness of the lunar disc, and the relative brightness of the corona, as due simply to the fact that the Moon is an opaque body very much nearer to us than the corona.

Let me renew my statement that it is the importance of the approaching eclipse which forces me to urge now views which I have long entertained. It appears to me that if, as I hold to be the case, the evidence respecting the corona is amply sufficient to prove it to be a solar appendage, then it would be a serious misfortune if any observers were to devote their time to establishing this fact. Instead of this, I should be glad to see every moment of the short duration of totality devoted both by general observers and spectroscopists to the inquiry *what sort of a solar appendage* the corona may be. On this inquiry depend issues of the utmost interest and importance to science; the other would be a waste of time: on one question we have abundant evidence; on the other (to quote the just words† of Professor Pritchard), "wise astronomers profess their profound ignorance."

* Mr. Lockyer tells me that M. Faye expressly suggests that there is some action at the Moon, and that, according to his and M. Faye's theory, it is thus the atmosphere gets illuminated. What the nature of the action may be I have not yet heard, nor can I conceive of any which would account, however roughly, for observed facts. Mr. Seabroke's paper on the Corona in the last number of our *Notices*, assumes no such action to take place; yet it is supposed that he is there defending Mr. Lockyer's theory. On the other hand, let me note in passing, Mr. Seabroke deals with an imaginary (I had almost said impossible) eclipse, and is therefore evidently not attacking my views respecting real eclipses. His arguments are, however, mathematically accurate. At the very moment and place of second and third contacts in an ordinary total eclipse, or during the occurrence of an exact total eclipse (which could last of course but an instant), the results he educes would doubtless take place, though it would be wholly impossible to observe them. But his formulæ are wholly inapplicable to the circumstances of any actual eclipse.

† Just *per se*, though somewhat too magisterially applied as a warning to myself. I also profess complete ignorance as to the nature and condition of the material forming the corona, but it is on account of that ignorance that I am so anxious to see the skilled observers of the approaching eclipse employing the opportunity in the most advantageous manner.